Pre-analysis plan: Business Games for farmers and micro-entrepreneurs in Zambia*

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1 Introduction

The livelihoods of the majority of the population in developing countries depend on employment in small businesses and smallholder agriculture, but the incomes derived from these activities are often inadequate and volatile. Many farmers and micro-entrepreneurs lack the necessary management knowledge and skills to manage their businesses more successfully. Two thirds of the self-employed in Africa are classified as undereducated for their occupation, the highest share of all world regions (African Union Commission and OECD, 2024).

We propose a rigorous impact evaluation of the Business Games (BG), a simulation-based, three-day training program for farmers and micro-entrepreneurs, in Zambia. The features of the intervention allow our study to touch on multiple gaps in the literature on skills training programs and questions of high policy relevance. First, a recent review of skills training RCTs conducted recently in sub-Saharan Africa found that they are clustered in relatively few countries, with only two having been implemented in southern Africa and none in Zambia (Beber et al., 2024). Given that effects appear to vary substantially across contexts (Kluve et al., 2017; Ibarrarán et al., 2019), this remains a notable gap in the literature.

Second, while the existing literature has consistently found that more intensive programs yield more sustained impacts (Beber et al., 2024), very short training programs of less than one week continue to be implemented widely in development assistance. The advantages of these very short programs lie in their low implementation cost, allowing higher numbers of beneficiaries to participate, and the low opportunity costs in terms of beneficiaries' lost income. Identifying new, innovative, and effective low-cost interventions is thus a highly pertinent policy and research imperative. The BG aspire to be such an innovative intervention.

Third, the BG are based on the psychologically well-founded experiential learning approach, which to our knowledge has not yet been the object of an RCT in the context of skills trainings in sub-Saharan Africa. Most trainings in this field have traditionally been classroom-based, but innovation and pluralism in techniques abound in newer programs. Examples of such innovative approaches include visits by role models (Lafortune et al., 2018), personal initiative training (Campos et al., 2017), peer learning (Dalton et al., 2021), or individual coaching sessions (Bardasi et al., 2019). The experiential learning approach is another such innovation.

2 Study design

2.1 Experimental intervention

The **Business Games (BG)** are simulation-based learning formats focusing on business and financial knowledge that were developed by the German Sparkassenstiftung for International Cooperation (DSIK) and are implemented with different target groups around the world. DSIK generally works with local implementing organizations and uses a train-the-trainer approach to qualify trainers for their task. This rigorous impact evaluation analyses the effects of the **Farmer Business Game (FBG)** and the **Micro Business Game (MBG)** in Zambia, where they are implemented in cooperation with local non-governmental organizations (NGOs). The interventions that are part of this study are implemented in four provinces of Zambia (Northwestern, Copperbelt, Eastern, Muchinga) by five NGOs: the New Apostolic Church Relief Organization, NACRO; Youth Alive Zambia; Charity Centre for Children and Youth Development, CCCYD; Supernova; Zambia Federation of Associations of Women in Business, ZFAWIB.

Both the MBG and the FBG have a duration of three days and consist of so-called Know-How Sessions, where participants learn key management and financial education concepts, and multiple simulation rounds. During the simulation rounds, the participants work in teams to manage a (farm or non-farm) business and compete with the other teams. At the end of each round, the participants receive feedback and discuss the outcome of the round based on their management decisions, such as opting for a certain crop, investing in business assets, or deciding to purchase or forego insurance.

The main objective of the study is to assess the impact of the BG on key outcomes of beneficiaries along the results chain. These range from a) knowledge (business and financial knowledge), over b) access to finance and financial behavior, c) business and farming practices, to d) economic well-being and employment (measured in terms of business and non-business income, assets, as well as job creation).

- **Content and topics covered:** The surveys include sections on respondents' basic demographics and employment status, farm/business characteristics, management practices, agricultural production and business sales, income and assets, access to finance, cognitive skills, and empowerment. The data we use and collect does not cover sensitive topics, and the data collection does not involve any

invasive or harmful procedures.

- **Mode of survey administration:** Baseline and follow-up survey data are collected in person by a team of enumerators using a tablet-based questionnaire, programmed using the software SurveyCTO, in training locations or at meeting points, throughout Zambia.

2.2 Survey sampling

The study is implemented as a randomized control trial using pre- and post-intervention survey data.

1. We conduct a **baseline survey with structured interviews of about 1,540** individuals in at least four provinces of Zambia.

2. In addition, we use a **financial decisions questionnaire**, a brief knowledge test conducted after the intervention (for the treatment group), or after the baseline survey (for the control group).

3. We conduct an **endline survey with the treated individuals as well as those in the control group, in 2025.**

Research participants are suggested and recruited by local groups and associations (for example village-level groups, cooperatives) that expressed an interest in organizing an edition of the BG in their community. Contact with these groups is brokered by the NGOs who implement the BG.

The individuals who participate in the study are thus interested and eligible to participate in the BG.

These NGOs who implement the study identify groups across the four provinces (for example cooperatives, youth associations, women's associations) that have expressed an interest in organizing an edition of the BG for their members. While each of these groups recruits potential participants of the BG, the recruitment process and eligibility criteria vary somewhat between the NGOs in line with their differing target groups. In some cases, recruitment focuses on group members. In other cases, recruitment is broader and involves village leaders. In terms of eligibility criteria, participants are generally required to be established or emerging farmers (FBG) or entrepreneurs (MBG). In the case of membership-based organizations, membership in a group may

be a requirement to participate in the Game. Some NGOs also ask that all or most of the group know how to read and write. Participants have to be at least 18 years old.

2.3 Treatment assignment

The NGOs schedule BG in specific locations in September and October 2024. They provide a list of groups, with a pair of two potential groups for each planned BG in a given location. One group for each pair is then randomly assigned to the treatment group (participation in the BG in 2024), and the other to the control group (participation in 2025). The study covers all potential participants as recruited by the NGOs according to the procedure(s) described above.

2.4 Sample sizes and power

- **Number of clusters:** 70
- **Number of observations:** 1,540
- **Sample size by treatment arms:** 770
- **Minimum detectable effect sizes for main outcomes:** δ=0.07, assuming 70 clusters with 22 participants each, power of 0.8. Further assuming that the intra-class correlation $\rho = 0.15$,[1] δ corresponds to about 15% of a standard deviation (see Table 1.)

[1] This assumption is based on estimated intra-class correlations for selected outcomes using baseline data, ranging from 0.07 for having a bank account, to 0.15 for financial literacy.

alpha	power	K	M	N	delta	m0	ma	sd	rho
.05	.8	70	22	1,540	.07139	0	.07139	1	0
.05	.8	70	22	1,540	.07139	0	.1257	1	.1
.05	.8	70	22	1,540	.07139	0	.1454	1	.15
.05	.8	70	22	1,540	.07139	0	.1628	1	.2
.05	.8	70	22	1,540	.07139	0	.1929	1	.3
.05	.8	70	22	1,540	.07139	0	.2189	1	.4
.05	.8	70	22	1,540	.07139	0	.2421	1	.5
.05	.8	70	22	1,540	.07139	0	.2633	1	.6
.05	.8	70	22	1,540	.07139	0	.2829	1	.7
.05	.8	70	22	1,540	.07139	0	.3012	1	.8
.05	.8	70	22	1,540	.07139	0	.3185	1	.9

Table 1: Minimum detectable effect size and alternative means for different values of intra-class correlation.

2.5 Hypotheses

We formulate four primary hypotheses concerning the treatment effects on individual-level outcomes.

H1 Knowledge acquisition in terms of financial literacy and management knowledge. These outcomes are measured for the first time immediately after the end of the intervention for the treatment group, and directly after the baseline questionnaire for the control group. They are measured a second time during the follow-up data collection around nine months after the intervention. We generally expect members of the treatment group to have a better understanding of these concepts after having participated in the intervention. The effect measured immediately after the intervention shows to what extent participants grasped the contents of the training and retained them in the short term. The effect measured about nine months after the intervention show the sustainability of this learning effect.

H2 Access to finance and sound financial behavior: These outcomes are assessed approximately nine months after the intervention. We hypothesize that individuals in the treatment group have better access to finance and more sound financial behaviors after the intervention. More specifically, we expect them to be more likely to have a bank account, have higher savings, be more likely to save up for a concrete saving goal, and be more likely to have loans and insurance. In terms of loans, we expect them to have better loan conditions, have more productive loan use, and have a lower likelihood of over-indebtedness.

H3 Business and farm management: We hypothesize individuals in the treatment group to be more aware of the benefits of management techniques such as appropriate record-keeping and planning. As a result, we expect to find higher rates of these practices in the treatment group. In terms of agricultural decision-making for farmers, we expect individuals in the treatment group to use their improved knowledge for sound decision-making in terms of crop selection, have higher rates of beneficial practices like crop rotation and diversification. We also expect treatment group farmers to have higher rates of cooperative membership, higher market orientation, and obtain higher prices for their crops.

H4 Economic well-being and employment: We hypothesize that individuals in the treatment group have higher incomes after the intervention, an effect that is likely driven by improvements in the outcomes listed under H1), H2) and H3). We may also see increases in assets, as a consequence both of higher incomes and a higher propensity

to invest. Business or farm growth could allow treatment group farmers/entrepreneurs to hire more employees and offer them improved working conditions.

We will additionally consider the following secondary hypothesis, which we may analyze separately from the hypotheses above and discuss in a separate study output.

H5 Empowerment: We hypothesize that individuals – more specifically, female individual engaged in farming activities – in the treatment group have a higher degree of empowerment measured by using selected questions from the Abbreviated Women's Empowerment in Agriculture Index (A-WEAI).

2.6 Outcome measurement

Included below is a list of core outcome measures for each of our hypotheses. If an outcome does not meaningfully vary in our sample, we report this and drop the outcome from further analysis. In some cases, an outcome measure's value is implied by an item's relevant survey logic, and for readability's sake we do not list all logic-relevant variables as such in the table below.

- **H1 Knowledge acquisition** in terms of financial literacy and management knowledge. These outcomes are measured for the first time immediately after the end of the intervention for the treatment group, and as part of the financial decisions questionnaire directly after the baseline questionnaire for the control group. They are measured in a reduced from a second time during the follow-up data collection nine months after the intervention.

  a) **financial literacy**, index following OECD (2022).

  b) **sound financial decision-making**, an index based on responses to hypothetical scenarios.

  c) **accounting knowledge**, knowledge of key accounting concepts.

- **H2 Access to finance and sound financial behavior:** These outcomes are assessed approximately nine months after the intervention. Specific outcomes include

  a) **bank account:** having a bank account.

  b) **saving:** total savings, saving frequency, saving goal.

c) **loans:** having a loan, loan sustainability, loan usage.

d) **insurance:** different types of insurance.

- **H3 Business and farm management:**

  a) **Record-keeping:** keeping records of sales, expenses, income statement, balance sheet, cash flow statement.

  b) **Planning:** sales forecasting.

  c) **Agricultural decision-making:** crop selection, crop rotation, diversification, input use.

  d) **Networks and market orientation:** cooperative membership, market orientation and sales prices.

- **H4 Economic well-being and employment:**

  a) **Income:** business, personal, and household income.

  b) **Assets:** livestock and other agricultural and assets, household assets.

  c) **Own employment:** own employment status and business start-up, job satisfaction.

  d) **Employment of others and employment conditions:** no. of employees, existence of written contracts for employees, wages.

- **H5 Empowerment:**

  a) **Female empowerment in agriculture:** Selected questions from the A-WEAI Index

  - Making decisions
  - Input in decisions
  - Freedom to make own personal decisions

2.7 Heterogeneous effects

We report heterogeneous effects for each outcome and treatment-control group comparison, either by using separate samples or interactions. We report such effects for

the different training providers and the type of training (FBG vs. MBG), as well as key respondent characteristics (e.g. gender, literacy and/or education, language skills) and baseline values of the dependent variables.

2.8 Timeline

- **August 2024**:
  - Recruitment and training of consultant and enumeration team.
  - Kick-off workshop with stakeholders.
  - Random assignment of treatment and control group.
- **September - October 2024:** Baseline data collection and implementation of the BG
- **From January 2025:** Analysis of baseline data and the impact of the treatment on skills acquisition (financial literacy and business knowledge).
- **March 2025:** Stakeholder workshop with implementing partners.
- **July - August 2025:** Endline data collection.
- **From October 2025:** Analysis of endline data and preparation of reports and manuscripts.

3 Analysis plan

3.1 Core specification

We generally estimate treatment effects on our outcomes of interest using an **intent-to-treat** approach, comparing subjects assigned to participate in the BG to those in the control group. That is,

$$Y = \alpha + T\theta + X\beta + \varepsilon, \tag{1}$$

where Y is an outcome of interest, T is a treatment indicator, θ our estimand, and X a set of covariates, which includes a pre-treatment measure of the relevant outcome variable, if applicable; strata and enumerator fixed effects; and additional control variables in case of imbalances between treatment and control variables.

- In the case of **missingness in covariates**, we impute mean or zero values and use the missingness-indicator method, as described in Zhao and Ding (2024).
- We report **clustered standard errors** as needed given clustered treatment assignments.
- We report the results of **one-sided t-tests** for hypotheses that state an effect direction, and two-sided tests otherwise.
- We do not formally adjust for **multiple hypotheses testing**, but transparently display the total number of tests we carry out within each block of outcomes.
- If treatment delivery is incomplete, we report results from an **instrumental variable regression** using assigned treatment as an instrument for actual treatment delivery.

3.2 Randomization checks

We **report the extent to which observable covariates are balanced** across treatment conditions, as expected. Relevant baseline variables include gender, age, marital status, household size and number of children, literacy, education level, individual and household income, household wealth, as well as baseline-available outcome measures.

3.3 Effect predictions

We **benchmark estimated effects against prior beliefs** elicited from stakeholders.

3.4 Compliance

In the treatment group, we do not observe non-compliance in terms of invited individuals not participating in the BG, as our baseline sample consists of those present on the first training day. We also do not expect non-compliance with treatment assignment in the control group, as participants were recruited personally within their groups, and control group data collection mainly took part during the implementation of the BG.

There might be instances of partial compliance in the treatment group, however, where participants are not there for all training days. We **check for differences between units that are fully and those that are partly in compliance with their experimental assignment**, and report mean differences and associated statistics for the same set of baseline variables and baseline-available outcome measures listed

in section 3.2. In the case of substantial partial compliance, we report instrumental variable estimates for the specific comparisons and outcomes, with random treatment assignment serving as an instrument for actual treatment.

3.5 Attrition

We **compare non-attrited and endline-attrited subjects** by computing mean differences and associated statistics by treatment status and for baseline variables and baseline-available outcome measures as in section 3.2. We report trimming bounds (Lee, 2009) if needed to address substantial attrition.

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